Levels of explanation in biological psychology

Looren De Jong, H.

published in
Philosophical Psychology
2002

DOI (link to publisher)
10.1080/0951508021000042003

document version
Publisher's PDF, also known as Version of record

citation for published version (APA)
Looren De Jong, H. (2002). Levels of explanation in biological psychology. *Philosophical Psychology*, *15*, 441-462. https://doi.org/10.1080/0951508021000042003

Philosophical Psychology
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/cphp20

Levels of explanation in biological psychology

Huib Looren de Jong

Version of record first published: 19 Aug 2010.

To cite this article: Huib Looren de Jong (2002): Levels of explanation in biological psychology, Philosophical Psychology, 15:4, 441-462

To link to this article: http://dx.doi.org/10.1080/0951508021000042003

PHILOSOPHICAL PSYCHOLOGY, VOL. 15, NO. 4, 2002

Levels of explanation in biological psychology

HUIB LOOREN DE JONG

ABSTRACT *Until recently, the notions of function and multiple realization were supposed to save the autonomy of psychological explanations. Furthermore, the concept of supervenience presumably allows both dependence of mind on brain and non-reducibility of mind to brain, reconciling materialism with an independent explanatory role for mental and functional concepts and explanations. Eliminativism is often seen as the main or only alternative to such autonomy. It gladly accepts abandoning or thoroughly reconstructing the psychological level, and considers reduction if successful as equivalent with elimination. In comparison with the philosophy of mind, the philosophy of biology has developed more subtle and complex ideas about functions, laws, and reductive explanation than the stark dichotomy of autonomy or elimination. It has been argued that biology is a patchwork of local laws, each with different explanatory interests and more or less limited scope. This points to a pluralistic, domain-specific and multi-level view of explanations in biology. Explanatory pluralism has been proposed as an alternative to eliminativism on the one hand and methodological dualism on the other hand. It holds that theories at different levels of description, like psychology and neuroscience, can co-evolve, and mutually influence each other, without the higher-level theory being replaced by, or reduced to, the lower-level one. Such ideas seem to tally with the pluralistic character of biological explanation. In biological psychology, explanatory pluralism would lead us to expect many local and non-reductive interactions between biological, neurophysiological, psychological and evolutionary explanations of mind and behavior. This idea is illustrated by an example from behavioral genetics, where genetics, physiology and psychology constitute distinct but interrelated levels of explanation. Accounting for such a complex patchwork of related explanations seems to require a more sophisticated and precise way of looking at levels than the existing ideas on (reductive and non-reductive) explanation in the philosophy of mind.*

1. Introduction

Some concern for the future of psychology as a discipline seems in order. Neuroscience, genetics, psychopharmacology, and evolutionary biology are making inroads in the traditional domain of psychology, the explanation of behavior. According to some, psychology may thus face annexation of its subject (human behavior and mental life) by the different branches of biology. Many philosophers of mind have nevertheless argued that psychology has its own identity, residing in the higher-level,

Huib Looren de Jong, Vrije Universiteit Amsterdam, Department of Psychology, Van der Boechorststraat 1, 1081 BT Amsterdam, The Netherlands, email: h.looren.de.jong@psy.vu.nl

ISSN 0951-5089/print/ISSN 1465-394X/online/02/040441–22 © 2002 Taylor & Francis Ltd
DOI: 10.1080/0951508021000042003

mentalistic and functional, vocabulary of its explanations. In this paper some of the main options for relations between psychological and biological (genetic, neurophysiological) theories are sketched, and a recent showpiece of biological psychology (behavioral genetics) is briefly introduced. Since biological psychology is typically an interlevel enterprise, we will look for models for interlevel relations in philosophy of psychology, as well as in philosophy of biology. Surprisingly perhaps, philosophy of biology seems to offer more resources for understanding relations between levels. We will argue that the traditional ideas on reduction in the philosophy of science are too simple, and that in coming to grips with the way this kind of science is really done, we need more sophisticated models for interlevel relations. Apart from the issue of which models of reduction are illuminating in understanding actual explanatory practices in science, we will also briefly look at some more metaphysically and/or normatively motivated concerns about constraints on interlevel explanations.

2. Varieties of interlevel relations in neuroscience and psychology

2.1. Classical reduction

The classical account of reduction follows from the deductive–nomological (D-N) model of explanation (Hempel, 1965; see also Nagel, 1961). It holds that explaining an event involves deducing it (more precisely, a statement describing it) from a theory plus boundary conditions; likewise, reduction is deducing a higher-level theory from a lower-level theory plus boundary conditions. What constitutes a higher level is not very clear, but the background ontology assumes that higher levels are characterized by more complex entities, that complex entities are composed of simpler entities, and that the former are explained by reducing them to their elementary constituents (see Oppenheim & Putnam, 1958/1991, for the relation between microreduction, decomposition and the notion of levels; for the idea of working one's way back to the constituents as explanation in physics—the "arrow of reduction"—see Weinberg, 1993).

Nagel (1961, pp. 354, 433–435) gives two conditions for theory reduction: connectability and deducibility. The deducibility condition implies that theories are finished and formalized. Bridge laws take care of the connectability condition by establishing cross-theoretical identities. The textbook example of a bridge law is: temperature is average kinetic energy of molecules in an ideal gas. After connecting the vocabularies of the two theories through such bridge laws, the (formalized) theory of thermodynamics can (ideally) be deduced from (i.e. reduced to) statistical mechanics, its concepts will map nicely onto those of the reducing theory, and its ontology can be retained.

Unfortunately, these conditions are almost never realized. When a theory is reduced, the meaning of its terms is usually changed in the process. A standard example is the difference in meaning associated with the concept of mass in Newtonian and in quantum physics, respectively. This makes identification and the formulation of bridge laws impossible. Moreover, since the reduced theory is usually in some way or other corrected by the reducing theory, the former can strictly

speaking not be logically consistent with the latter. Assuming that the reducing theory is correct, the reduced theory must have been at least partially false, and therefore cannot be deduced from the former (it is logically impossible to deduce a false statement from a true statement). The classical (Nagel) reduction model assumes consistency and meaning invariance between reduced and reducing theory, and as was realized early in the history of modern philosophy of science, meaning change and corrections are essential for empirical progress (Feyerabend, 1968). As Kuhn (1970) famously argued, after scientific revolutions worldviews change and ontologies are abandoned. Furthermore, the kind of finished and formalized theories required for deduction are extremely rare.

Of course, the intention of the D-N model was to provide a rational reconstruction of scientific explanation. The model belongs to the context of justification, not to the context of discovery, and it could be argued that the scarcity of real life instances does not compromise the ideal case (cf. Schaffner, 1993, p. 496, who suggests that a unilevel clarified science may be hoped for, for which the gappy and bushy connections in interlevel fragmentary sciences may be precursors). However, the fact remains that the classical view of reduction does not give a plausible picture of scientific practice and scientific progress: the framework it proposes as the ideal type of reduction does not fit obvious cases of progress and successful reduction, and does little to clarify what constitutes reductive success. The consequences of this failure are interesting.

Broadly speaking, two quite different morals for the future of psychology can be drawn from the failure of the classical reduction model. The impossibility of establishing cross-theoretical identities (expressed in bridge laws) between mental and physiological processes was hailed as a virtue by early cognitive psychology, since it presumably guaranteed the irreducibility, and thereby the autonomy for psychology *vis-à-vis* neuroscience (Fodor, 1981a). More precisely, the domain of psychology was identified as that of multiply realized functions, and multiple realization supposedly precludes bridge laws. The other response was eliminativism (Churchland, 1981), of which New Wave Reductionism (Bickle, 1998) is the latest and most sophisticated offshoot. This tries to incorporate the failure of the connectability and deducibility conditions, i.e. the need to correct the reduced theory, in a more sophisticated model of intertheoretic reduction (Churchland & Churchland, 1994; Schaffner, 1993).

3. Multiple realizability and its problems

Until recently, the notions of function and multiple realization were supposed to save the autonomy of psychological explanations. Fodor (1981a,b, 1990) and others have argued that as long as intentional laws can be found that explain and predict behavior, their relation with underlying neurophysiology is in principle not relevant for explanation. Mental processes are understood as functions with a specific causal role (e.g. hunger causes foraging behavior), and, like programs in computer science, this can be multiply realized. Hunger in octopuses is a different neural state from hunger in man, but that does not matter for understanding its causal role, which

may be relevantly similar (Putnam, 1961) (perhaps even silicon chips may be said to be hungry if they exhibit a certain functional state that produces food-seeking behavior). The hardware (or neural wetware) is merely implementation. The core business of classical reductionism, identifying mental functions with their neural realizers, would obscure the functional generalizations that constitute psychological theory. Functional and intentional laws would be invisible in the physiological machinery (Fodor, 1981b), in the same way as the logical structure of a computer program would be invisible in the physical states of the machine. Furthermore, it would go unnoticed how different neural systems may serve the same functions. Thus, on this view, psycho-neural reduction by identifying mental and physical events is not only impossible but also undesirable.

This amounts to methodological dualism (or, potentially, pluralism): functional psychology has its own vocabulary, laws and explanations, which are distinct from physics. That is not to deny that every function is realized somehow in some material substrate.

3.1. Supervenience

The concept of supervenience (Kim, 1993) captures both this ontological dependence of the mind on the brain, and the methodological autonomy of psychology. Supervenience holds that individuals that are indiscernible with respect to their physical properties are indiscernible with respect to their mental properties. However, it does not exclude that mental properties may have different realizers. Thus, the covariance of mental and physical properties is asymmetrical. The dependence of mind on brain is reconciled with the non-reducibility of mind to brain; supervenience combines materialism with an independent explanatory role for mental and functional concepts and explanations—briefly, with methodological dualism.

Multiple realization does not preclude the discovery of psychophysical connections—a functional state may be associated with an open disjunction of many distinct physical states. The crux is that these are not (bridge-) lawlike. Presumably, an ontologically heterogeneous collection of realizers is incompatible with orderly bridge laws connecting natural kinds (Kim, 1992). Multiple realization thus provides firm ground for non-reductive materialism: the functional states that are characteristic of psychology are physically realized, but the unavailability of bridge laws blocks classical theory reduction.

3.2. Local reductions

Recently, however, Kim (1998) has questioned the feasibility of non-reductive materialism, and argued that functionalism properly understood implies a reductive relation between psychology and neurophysiology. Kim argues that supervenience, non-reductive materialism, and multiple realization do not legitimate an autonomous higher level of mental or functional explanation. His metaphysical point of departure is that only entities with causal powers are to be taken serious as potential

explanations ("to be is to be causally effective"). If we assume that the physical world is causally closed (the alternative would be that some spooky force could intervene in physical processes), then there simply is no room for independent mental causation, and hence no room for other than physical explanation. The use of functional characterizations is to define the explanandum, the role a phenomenon has, but this is only the first step towards real explanation—which, in Kim's view, must be causal explanation. The next step is finding the realizers (in the case of mental processes, the underlying neural processes), and developing a theory how these produce the phenomenon.

Kim calls this the functionalizing strategy, and claims that is the way science works. For example, pharmacology defines the functional role of a substance as dormitivity, and then looks for the chemical substance that realizes that role, and tries to find out why it has that specific effect. Functionalization amounts to reduction. Functions have no causal powers, hence no (genuine, metaphysically respectable) explanatory powers; functional states are roles, filled by physical states, they are the second order effects of a first order that is ultimately, really physical, and are to be explained in physical terms. So, a successful reduction is not as Nagel thought, the derivation of higher order laws form lower order plus boundary conditions. Kim rejects the classical view of (Nagel type) reduction. No bridge laws are available, and even if there were, these would not explain anything, but just state correlations that may well be compatible with dualism or emergentism. Hence, not even ontological simplification would result from traditional reduction via bridge laws between mental and physical terms. Functionalist reduction explains bridge laws as identity of second order and first order properties, whenever the physical mechanism fits the mental description.

In Kim's view, the real causal-explanatory work is done at the level of the realizers. What seems like multiply realized properties (as in intentional laws in psychology) is really a disjunctive set of physical properties. When for some mental or functional property Putnam–Fodor-style functionalism applies, then this multiply realized higher-level property must be causally heterogeneous: having different realizers, it generalizes over a disorderly jumble of physical causes. Therefore, it lacks physical and hence explanatory integrity. It may well be, Kim suggests, that the (functional) concepts of psychology do not refer to real kinds in nature, and that, since we want only ontologically sound natural kinds in explanations, they are better dispensed with, and replaced by the real neural properties. Then the real explanation is in local, perhaps species-specific, reductions of the functional mental properties to each of its distinct realizing neural properties.

Thus, Kim pictures a kind of reduction without bridge laws, and gives some serious arguments for rejecting multiple realization arguments for autonomy. It should be noted that whereas Putnam and Fodor seem happy to restrict their defense of the autonomy of functional explanations to arguments from the philosophy of science (the absence of bridge laws and the impossibility of classical reduction), Kim introduces metaphysical concerns: the causal impotence of higher-level causes precludes in his view a legitimate explanatory role.

3.3. Heuristic versus reductionist identification: pluralism

Thus, Kim gives the multiple realization argument a reductionist turn: we should abandon functional generalizations as non-explanations and look for the local reductive relations. However, even if we agree that classical functionalism is in danger of conflating empty "cheap" generalizations (Churchland, 1981) with real explanatory laws, and that autonomy has a stifling effect on progress in neuropsychology, the reductionist conclusion does not follow. Like Kim, McCauley and Bechtel (2001) and Bechtel and Mundale (1999) reject multiple realization arguments for autonomy, and they advocate (heuristic) identification of mental and physical properties. However, their argument is both more subtle and more empirical than Kim's. They argue that this (heuristic) identification is relative to the level of detail (coarseness of grain size). Grain size is a context-dependent and theory-relative, ultimately empirical, affair. For example, whether hunger in an octopus is the same as in humans or Martians depends on the level of analysis. It may be an excellent research question how differences in neural make-up explain difference in the functional properties of hunger in different species. This is the crux of heuristic identification (Heuristic Identity Theory; see McCauley & Bechtel, 2001): a tentative bridge between two sets of theories, most typically between those of neuropsychology and those of functional or cognitive psychology.

Bechtel and Mundale (1999) show that, in animal studies, neuropsychologists have always exploited the homology between nervous systems of different species to draw conclusions about the functions of human brain areas, and multiple realization is no barrier to successful identification of cognitive functions with brain structures across different species. The interesting consequence is that from the first assessment that a putative functional or psychological property is multiply realized we can proceed in opposite ways: either towards fine-grained local reduction, or towards broad, abstract functional generalizations. Sometimes, we may want broad generalizations, abstracting from implementational details (for example, when we lump together hunger in different species as having the same causal role); sometimes we may want local narrow reductions, individuating different neural mechanisms in different species as realizing different functional (mental) properties; and sometimes somewhere in between splitting functions and lumping them—to borrow a phrase from Sober (1999).

So, (heuristic) identification is more pluralist than Kim seems to realize: it depends on explanatory interest and grain size. There is more to finding identities in cognitive neuroscience than the unique causal-ontological rock bottom Kim demands; identification is relative to multiple explanatory interests. As we will argue below, the pluralist view on identification seems to fit interlevel contexts better than Kim's functionalizing/reductionist view.

4. New Wave Reductionism

In the parting of ways after the failure of classical theory reduction, eliminativism (and its latest incarnation, New Wave Reductionism) has been the main (or only)

alternative to autonomy (Bickle, 1998; Churchland, 1981). It gladly accepts abandoning or thoroughly reconstructing the psychological level, and considers reduction if successful as equivalent with (potential) elimination and replacement by neuroscientific concepts and explanations. Churchland writes: "... what a successful reduction shows is that one way of conceiving things can be safely, smoothly, and—if the excess empirical content of T_n over S_n is corroborated—profitably *displaced* by another way of conceiving things. And this, I submit, is the function of reduction. *A successful reduction is a fell-swoop proof of displaceability*; and it succeeds by showing that the new theory contains as a substructure an equipotent image of the old" (Churchland, 1979, p. 82; italics in original).

Bickle (1998, 2001) is one of the most articulate defenders of the thesis that neuroscience will replace psychology. Interestingly, he presents New Wave Reductionism (NWR) as an empirical bet on the outcome of the success of replacing cognitive or folk-psychological theories by neuroscientific theories. This makes it interesting to evaluate NWR as an account of our behavioral genetics case study below.

NWR presumably solves the problem of elimination versus smooth reduction. An early attempt in this direction was made by Schaffner (see his 1993, Chapter 9), who proposed to construct a corrected version of the to-be-reduced theory, that can then be deduced from the new reducing theory. A similar proposal was made by Hooker, the difference with Schaffner's being that Hooker proposed to formulate the corrected theory ($T_R^\star$) in the vocabulary of the new reducing theory (T_B), whereas in Schaffner's proposal it was to be drawn from the old reduced theory (T_R). The Hooker theory and the Schaffner theory were both intended as answers to a particular nasty problem with the connectability condition, namely, that the reduced theory is usually false, and a false conclusion can not be logically deduced from a true theory (the reducing theory). Bickle (1998, 2001) presents his theory as an extension of Hooker's; in his version of NWR, no connecting principles (bridge laws) are needed, since the vocabulary of the corrected theory is already part of the reducing theory. NWR thus solves the connectability and derivability problems. The price to be paid for this innovation is that strictly speaking the old theory as such is not reduced, but replaced—or at least rewritten.

A novelty introduced by the new model of reduction is that the degree of correction with the old theory is now an important parameter of the reduction. This may range from almost perfect retention (smooth reduction) to complete rejection (bumpy reduction) at the other extreme; in ontological terms, the continuum ranges from perfect retention to total replacement of a theory's ontology. The identification of temperature with mean kinetic energy is an example of smooth reduction, where the thermodynamic concepts map almost completely onto those of statistical mechanics, and the phlogiston theory of combustion, which was entirely replaced by the oxygen theory, is an example of bumpy reduction. The smooth reduction end corresponds with the D-N micro reduction with cross-theoretic identities (bridge laws) that guarantee connectability and derivability, the bumpy reduction end with Kuhnian revolutions. In between are varying degrees of correction of the reduced theory (Bickle, 1998, p. 30; Churchland & Churchland, 1994).

4.1. How neuroscience reduces cognition

Bickle's (1998, 2001) showcase of the reduction of cognitive processes to neural ones is LTP, (Shors & Matzell, 1997), where in his reconstruction the replacement of the (functional) laws of associative learning by neural mechanisms has already been accomplished. This amounts in his view to intermediately bumpy reduction (Schouten & Looren de Jong, 1999, 1997). Bickle (1998) presents this as a case of revisionary physicalism: a genuinely cognitive theory reduces (with revision and perhaps "enrichment") to neuroscience. More precisely, he envisages no total elimination, but there will be significant conceptual change, and therefore no cross-theoretic or property identities in the reduction of cognitive psychology to neuroscience.

Quantitative predictions in cognitive vocabulary (like representation, information, redundancy, surprise) are smoothly mapped onto underlying mechanisms of intracellular mechanisms (presynaptic potentiation, released neurotransmitter, etc.). These mechanisms form the neural alphabet, and combinations and sequences of these (in combination with more complex neural wiring, interneurons, etc.) produce phenomena functionally characterized as cognitive. This is reduction in the sense that the ontology of the functional mathematical model of conditioning is related to the components underlying these functional phenomena (in this case, associative learning). It constitutes a nice case of combinatorial reduction: the simple intracellular mechanisms can, when properly combined, presumably yield more complex kinds of cognition.

Applied to the prospects of the reduction of folk-psychology, Bickle positions his NWR as an alternative to traditional options of autonomy (functionalism), identity theory (smooth reduction with cross-theoretic bridge laws), and eliminative materialism (giving up the traditional mentalistic idiom, and starting to talk about our fellow humans in terms of neuroscience; Churchland, 1981). He also rejects non-reductive materialism, including property dualism, as thinly disguised dualism.

4.2. Reduction and/or co-evolution?

Three characteristics mark a successful, reasonably smooth reduction (Bickle, 1998, 2001). First, the reduced theory approximates (gives a global, abstract, coarse-grained description of) the phenomena for which the reducing theory describes the real underlying dynamics. Second, each of the concepts of the reduced theory fragments into several concepts of the (more finely grained) reducing theory. Thus, the former are "structured" (i.e. associated with distinct composite processes of the reducing theory). Third, in revisionary cases, there are mutual constraints and feedback from reduced to reducing theory, and vice versa. Thus, revisionary cases display conceptual change, not complete retention nor complete elimination. Bickle's third characteristic of mutual feedback is somewhat surprising. According to NWR, the old concepts in an intertheoretic reduction are, to some extent, re-described in the superior vocabulary of the new theory; at the bumpy end of the continuum, the old concepts are entirely eliminated, at the smooth end they are

vindicated as a limited subset of the new theory (Churchland & Churchland, 1994, p. 48). Importantly, in all cases, the new reducing theory is the sole arbiter of whatever value the old concepts may have, and determines whether they deserve a place in new scheme of things. Such an asymmetrical relationship seems incompatible with real mutual feedback. Bickle also fails to give reasons why one would want to retain folk-psychological characteristics, even in revised (enriched, refined) guise, once we have the reducing theory that reveals the real underlying dynamics. (Kim would probably disapprove of such weak-hearted reductionism.) Nor is it clear how the (after all not replaced) higher-level concepts could have anything valuable to contribute in their supposed feedback to the lower level. This omission suggests a more subtle way of looking at interlevel relations than either autonomy or elimination.

4.3. Intertheoretic relations: diachronous and synchronous

As mentioned, after the failure of classical reductionism, two alternatives emerged: functionalism and New Wave Reductionism (Bickle, 1998; Churchland, 1981, 1989). The latter gladly accepts abandoning or thoroughly reconstructing the psychological level, and in his more radical moments Churchland considers successful reduction as equivalent with the possibility of elimination (in his less radical moments, he paints a picture of a slowly maturing marriage rather than a sudden takeover; Churchland & Churchland, 1994, p. 53).

Looking carefully, there is an ambiguity in this picture: the reduction–replacement continuum seems to conflate theory *succession* with theory *reduction*. At least conceptually, one might distinguish between a diachronic dimension of intertheoretic relations (theory succession, where an old theory is more or less corrected and replaced by or smoothly integrated in a new theory), from a synchronic dimension, where at the same point in time theories make contact, in ways that vary from classical micro reductions to coexistence of incompatible theories. In the latter case, there is no demand for replacement of the reduced theory in time (McCauley, 1996). Rather, the relation between levels can be described as selection pressure, mutually constraining theorizing at *both* levels. Although Bickle (1998) proposes a criterion for distinguishing genuine theory reduction from mere theory succession (pp. 30–32), this turns out to be a more or less formal measure of smoothness on the retention–replacement continuum (pp. 100–101), not a distinction between synchronous coexistence and diachronous succession.

To sum up, the traditional options of (smooth-to-bumpy) reduction and autonomy constitute too low-dimensional a conceptual space. Apparently, some sort of coexistence in time (synchronic) should be envisaged.

5. Explanatory pluralism

Explanatory pluralism (McCauley, 1996) has been proposed as an alternative to eliminativism on the one hand and methodological dualism on the other hand. It emphasizes that theories at different levels of description (e.g. psychology and

neuroscience) can co-evolve, and mutually influence each other, without the higher-level theory being replaced by, or reduced to, the lower-level one.

Heuristic Identity Theory (HIT) (McCauley & Bechtel, 2001) expands these ideas. It holds that hypothesizing heuristic identities between levels or domains is a way to construe interlevel relations. For example, McCauley and Bechtel (2001) show how in cognitive neuroscience, hypothesizing that some function in cognitive psychology can be identified with a neuroanatomical mechanism leads to progress in both fields. In their case the dorsal and ventral stream in the brain can be identified with location and object identification (the "where" and "what" system), respectively, in vision (in further research, the real story turned out to be much more complicated, leading to refinements and revisions in both theories). Heuristic identities are in some ways the opposite of the classical identities enshrined in bridge laws: heuristic identities are dynamic and revisable rather than finished, hypothetical rather than formalized—rather than results of finished research, they are directions for further investigation. Hypothesizing identities between levels thus may lead to intra-level modifications, adjusting either the upper-level psychological theory (in this example, about the function of the visual identification systems) or the lower-level theory (about the neuroanatomical mechanisms) or both. Thus, we see here the outline of a model of co-evolution that fleshes out the notion of synchronous coexistence of different levels of explanation.

Such ideas seem to tally with the pluralistic character of biological explanation. Below, we will look beyond philosophy of mind, to the philosophy of biology to illustrate and substantiate such synchronic coexistence of theories as advocated by explanatory pluralists. As we are interested in aspects of reduction and interlevel relations in psychology, biological psychology will serve as a case study, since it combines functional/psychological, neuroscientific and (most recently) genetic theories and explanatory schemes. In the case of behavioral genetics, theories from neurophysiology, personality psychology, and molecular genetics interact. Recent developments in molecular genetics have opened at least a possibility of relating the base pair sequence on the genome to personality characteristics. Such a relation is very complex and defies as yet attempts in the philosophy of science to lay down necessary and sufficient conditions for reduction. From the perspective of explanatory pluralism, we would expect to find many local and non-reductive interactions between biological, neurophysiological, psychological and evolutionary explanations of mind and behavior.

5.1. Pluralism in biology: a patchwork of laws, functions and interlevel theories

In comparison with the philosophy of mind, the philosophy of biology has developed more subtle and complex ideas about functions, laws, and reductive explanation than the stark dichotomy of autonomy or elimination. First, philosophers of biology are less concerned about laws, which are the core of D-N explanation and reduction: since evolution is contingent natural history, universal laws in biology are rare. Furthermore, the concept of function in biology is multidimensional (Mitchell, 1995): for example, in some explanations it includes the history of the selection of

a trait, while other explanations focus on a trait's current causal role in a system (see Sober, 1985). Schaffner (1993) and others have argued that biology is a patchwork of local laws, each with different explanatory interests and more or less limited scope.

This points to a pluralistic, domain-specific, and multi-level view of explanations in biology. Contrary to popular accounts of Darwinism as a comprehensive worldview, most philosophers of biology agree evolutionary biology is not an integrated hard science like physics (Brandon, 1990; Schaffner, 1993). For example, Brandon (1990, p. 134) writes: "… I do not think evolutionary theory is a theory at all. Rather, it is a family of theories (and goals, methods, and metaphysics) related in complex and ever-changing ways." The consensus seems to be that biology has no, or very few, real laws, that biological kinds are historical and contingent. Biology is largely "instrumental," i.e. it is disunified, consisting mainly of a toolbox of heuristic devices, it is not a set of truths (Rosenberg, 1994), and it deals in natural history (van der Steen, 2000), not in universal laws. It might be contended that in biological explanation the more interesting contrast is between mechanisms and laws, not between contingent and universal. Although much could be said about the "lawlessness" of biology, and the role of mechanistic explanation (Bechtel & Richardson, 1993) and causal regularities (Waters, 1998), for the present purposes I focus on the contingent, disunified and instrumental character of biology.

5.1.1. Laws and "empty" generalizations. A recent issue in the philosophy of biology is whether (evolutionary) biology has real laws, and the consensus seems to be that, maybe surprisingly, it hasn't. Van der Steen (2000) argues that natural history, not general and timeless laws, is the main ingredient of explanation. Natural history refers to non-universal claims with a low degree of generality, and thus contrasts with universal laws. Seemingly general concepts like "fitness" and "adaptation" are empty placeholders that have little or no empirical content, and only acquire their explanatory value when filled with natural history. A presumably general law like the Principle of Natural Selection (PNS) has no real empirical content. A core concept like fitness, in the sense in which it is used in population genetics, only states that there are some features that explain survival, but it does not yet say what these factors are; therefore it has little explanatory force. Only when one specifies which features in what animal in which environment contribute to survival does the notion get body, but then it loses its generality.

To quote Brandon (1990), PNS is a schematic law, and has "no empirical content of its own, that is, it has no biological empirical content" (p. 139), and is as such not testable. It is only testable when applied to concrete cases, with empirical details filling the empty, merely heuristic mould of adaptive thinking. But then it applies only to a specific population in a specific environment, and loses its generality. According to Brandon, PNS may be useful in structuring explanations, i.e. in setting up a search space, providing heuristics, but not as an explanation in itself.

Thus, at least some of the most basic generalizations in evolutionary biology are not classical laws, but empty generalizations, that must be filled in by domain-

specific mechanisms provided by other branches of biology. The generalizations are however valuable in setting the agenda, and defining the explanandum.

Furthermore, when these mechanisms are filled in, differences emerge. Considering the way evolution works, it would really be amazing if the details of the different applications of PNS were similar in an explanatorily interesting way. As Beatty has argued, evolution is a contingent process; therefore, it would be miraculous if there were a single mechanism underlying all the taxa of the evolutionary tree (Beatty, 1996). Beatty connects his idea, the *evolutionary contingency thesis*, explicitly with theoretical pluralism. The most plausible explanation for the lack of general laws may be the empirical fact of evolutionary contingency. There may simply be no single mechanism for natural selection, or for gene regulation, or for speciation that applies everywhere, but there may be a variety of mechanisms, a different one in different branches of the evolutionary tree. Sometimes, when a "frozen accident" such as the emergence and subsequent establishment of the genetic code occurs, a single law will apply to all taxa, but usually there will be parallel evolution, and hence no general laws, but rather a lot of domain-specific, non-universal mechanisms.

So, when going from higher-level functions to lower-level mechanisms, generalizations tend to fragment: they are realized by different mechanisms in different domains. This means that functionalism in Putnam–Fodor style is right in emphasizing the barrier multiple realization poses for reduction, but wrong in proposing function as an autonomous level of explanation.

5.1.2. The multiplicity of functional explanation. A related issue is that the concept of function, as it figures in adaptive functional explanation, has multiple meanings and multiple uses (Mitchell, 1995). Analyzing the central and controversial concept of function in the philosophy of biology, Mayr (1988) claims that there are two largely different fields in biology, which coexist alongside each other, using different concepts and methods: functional biology and evolutionary biology. Functional biology considers the operation and interaction of structural elements, and their contribution to the system (the "how" question). Evolutionary biology looks at selectional history (the "why" or "how come" question). In this way, several levels of explanation for the same event may be said to coexist, depending on time scale and explanatory interest. For example (simplifying Mayr's case), the cause of migration may be said to be the physiological mechanisms in the birds' brain noticing the shorter daylight span, or the evolutionary selection for species that get moving when food supply is short. Correspondingly, philosophers of biology distinguish several notions of function that serve different explanatory interests: some explanations focus on current and future systemic function, others on historical adaptation (Amundson & Lauder, 1994; Enc & Adams, 1992; Mitchell, 1995).

Likewise, Mitchell (1995) argues that dispositional and etiological functions serve distinct explanatory purposes: dispositional explanation answers the question how a trait contributes to survival in a certain environment, whereas the etiological explanation answers the question why a trait is present. She urges us to consider such explanation as doing different things, directed at different targets, providing

answers at different levels of abstraction. Several such explanatory projects may coexist (e.g. dispositional explanation identifies the function of a trait, etiological explanation explains why a trait is there). Explanations are specific for domains and/or explanatory interests. The multiplicity of function means that a unique and complete functional explanation is not likely to be discovered. The Putnam–Fodor tradition in the philosophy of mind that sets apart a self-contained level of functional explanation must be misguided: function means different things in different explanatory contexts.

To sum up, analysis of explanation in biology suggests that biologists do not consider functional analysis a complete explanation; only when the implementing mechanisms and the alternative solutions are specified do we have a candidate explanation. These mechanisms of course are domain-specific (Wouters, 1999): pluralism of explanation means pluralism of levels and grain sizes, and interlevel interaction between these. The filling-in of empirical details requires continuous border traffic between levels—brief co-evolution of theories at different levels, exerting selection pressure both ways.

5.1.3. The historicity of biological explanation. Rosenberg (2001) offers an interesting explanation why "biology is history (all the way down)": the concepts of functional biology reflect the process of natural selection operating on local conditions. Natural selection constantly changes these conditions; its history is a matter of moves and countermoves, an evolutionary arms race, where organisms change their own design space, and change the environment in which they and other organisms have to make their living. Therefore, the kinds evolutionary biology posits are also subject to historical change. Thus, biology is essentially historical; empirical generalizations refer to historical contingencies. Rosenberg (2001, p. 148) writes: "The principles of the theory of natural selection are the only real laws in biology. Beyond the bare theory of natural selection itself, the rest of biology is a set of subdisciplines historically conditioned by the operation of natural selection on local circumstances during the history of the Earth." The apparent generalizations of functional biology are really spatio-temporally restricted statements.

To sum up, beyond the locality of biological explanations, Rosenberg emphasizes the historicity and contingency of biological explanations; its generalizations and concepts may change when evolution moves along its contingent trajectory through design space (Dennett, 1995).

5.1.4. Middle range theories: the locality of biological explanation. The points made above suggest that biological explanations can be seen as located in the middle range (Schaffner, 1993), between universal physical and chemical laws on the one side, and universal but empirically empty (Brandon, 1990) evolutionary heuristics on the other side. Schaffner (1993, pp. 119–121, 519–530) argues that one should distinguish between two kinds of universality: the first refers to the presence of a trait in a more or less wide range of organisms, the other to the "same cause, same effect" universality of causal mechanisms. So, biology uses causal generalizations which may, unlike physical laws, have only limited distribution across populations. Biology

is in the business of finding generalizations of limited scope. The laws of physics are everywhere the same, but the same evolutionary function may be realized in different mechanisms in different domains: the range (or distribution) may vary as a matter of evolutionary contingency or natural history.

Biology has no general timeless laws, unlike physics. It has lots of domain-specific, context-dependent and historical generalizations. The scope of generalizations is almost always limited to some branch or other of the evolutionary tree, almost never general, exceptionless and timeless. Moreover, the implementation or realization relation between the concepts at different levels may also be historically conditioned and domain-specific. A function may be subserved by different mechanisms at different points in its odyssey through design space (e.g. exaptations—Gould & Vrba, 1998—are traits that have been put into service for another function than that for which they were selected). When the arms race between organisms changes, the function of mechanisms, and the relation between them and what they subserve also varies.

Keeping these ideas in mind will help us understand that in biology several domains can coexist synchronically. Darden and Maull (1977) describe cases where interlevel theories connect domains of inquiry. Explanatory extension is Kitcher's (1981) label for the way a theory from one domain can solve problems in another domain—for example, classical and molecular genetics remain distinct endeavors, but knowledge of molecular genetics can explain the mechanisms behind the behavior of Mendelian genes (however, see Waters, 1994, and Rosenberg, 1997, for a reductionist view on classical genetics). This seems a better model for biological psychology than NWR. Let us see how and why explanatory pluralism in biological psychology is bolstered by these ideas.

6. Biological psychology

6.1. Behavioral genetics: twins and the genome

A case in point is behavioral genetics, where genetics, physiology and psychology constitute distinct but interrelated levels of explanation. The traditional approach in behavioral genetics, twin research, has recently been supplemented by molecular methods. Traditionally, populations of monozygotic (MZ) and dizygotic (DZ) twins were compared, to obtain estimates of heritability coefficients for phenotypic traits (McGue & Bouchard, 1998; Plomin *et al.*, 1997), like intelligence (Plomin & DeFries, 1998), personality, etc. Since MZ are 100% genetic identical, and DZ 50%, and were raised in the same environment, comparing these siblings allows the partitioning of population variance on some trait into genetic, shared environmental, and unique variance (including noise). Typical results are that 50% variance in intelligence in a population can be attributed to genetic factors, and that this percentage is higher for elderly twins than for young ones. Path analysis makes possible more subtle multivariate and multifactor designs.

In addition to this approach, which is a matter of statistical modeling and estimation, more recently it has become possible to study loci on the genome more

or less directly. After spectacular successes in finding the loci associated with single gene diseases (Huntington's disease being a well known case), the search is now for loci that contribute to complex traits. Since complex polygenic traits (intelligence, temperament) are presumably determined by many genes (plus interactions between them, and epigenetic effects), one such locus may contribute only a small proportion of the variance. Typical results are that a polymorphism on exon III of chromosome 11 is associated with novelty seeking and that the proportion of shared variance is 10%. Headline-catching examples were the "gay gene," when gay males were found to have a higher than chance level (83% against 50% chance level) for an allele on the long arm of X chromosome (in the Xq28 region). As Hamer and Copeland (1998, pp. 195–197) emphasize, this result means very little as yet. However, it is an example of the general strategy of finding QTLs (quantitative trait loci) associated with complex polygenic traits, which seems to open the possibility to trace a causal path from genome to gene products (neurotransmitter receptors) to mind (intelligence, temperament, etc.).

6.2. Genes, dopamine and personality

For our purposes, the most interesting aspect of this work is the way levels and domains are related: loci on the genome (that is, base pair sequences, usually markers, sometimes also real genes) may be related to gene products like neurotransmitter receptors, and gene products are presumably related to personality, temperament and behavior. Two substances related to such gene products are serotonin and dopamine. Serotonin is associated with depression, anger, hostility, etc. (it has somehow something to do with Prozac, with intake of cholesterol, and is influenced by social rank in apes). Hamer and Copeland (1998, p. 103) interpret serotonin as a kind of punishment drug; it makes an organism feel bad, and that signals to the organism that something should be done about the situation. This interpretation nicely fits predictions by Cloninger that serotonin has something to do with a personality or temperament characteristic called harm avoidance. Harm avoidance is one of the four temperament domains of Cloninger's personality scale TPQ (Tridimensional Personality Questionnaire). (The other temperament domains are "novelty seeking," "reward dependence," and "persistence.")

Another example is dopamine. Recent research suggests that the number of repeats of a sequence of 48 base pairs on the *D4DR* gene that codes for the D4 dopamine receptor may be related to the ability to bind dopamine; that in turn seems to be related to novelty seeking (as measured on a questionnaire), and to risky behavior, and perhaps ADHD (attention deficit/hyperactivity disorder). If confirmed (it should be noted that some studies failed to confirm the association between the number of repeats and novelty seeking), these results would constitute a case of interacting and co-evolving levels of explanation.

As to the biochemical level, D4DR gene lies on the short arm of chromosome 11 (exon III) and codes for a dopamine receptor. Dopamine is something like a pleasure or motivation substance. It stimulates euphoria and exploratory behavior; dopamine deficiency is associated with immobility, as in Parkinson's disease, excess

of dopamine with exploratory behavior, and in extreme cases with schizophrenia. Dopamine is also associated with a dimension on Cloninger's personality scale TPQ dubbed "novelty seeking" (Cloninger *et al.*, 1996). Some of the neurochemical mechanisms are known, including pathologies (Parkinson), and substance abuse (like alcoholism), and the D4DR mRNA is clearly associated with the limbic system where emotional behavior is controlled (Ebstein *et al.*, 1996). The genetic polymorphism is the number of repeats of a minisatellite of 48 base pairs. Ebstein *et al.* (1996) found that the more repeats, the less responsive the brain is to dopamine, and the more novelty seeking that results. The personality type which scores high on the novelty seeking personality is impulsive, excitable, typically the bungee jumper or paratrooper, while low scorers are typically sedate and stolid, typically the clerk or librarian. The gene is also related to ADHD syndrome in children.

These results should be interpreted with caution. The shared variance, i.e. the percentage of behavioral variance that is accounted for by genetic variance, is typically in the order of only 10%. Hamer estimates that there must be hundreds of genes that influence behavior and personality, and the genes associated with serotonin and dopamine are only two of them. Also, there are major methodological and statistical problems in studying associations between genes and behavior. A number of studies failed to confirm Ebstein's results.

That being said, the interesting novel aspect of these results as compared with classical behavioral genetics is that now the biochemical level is directly studied, rather than only indirectly implicated, as in twin studies, and is related to the neurochemical level and to the psychological/personality level. Terms like "pleasure drug" or the "gay gene" are interlevel concepts in themselves. We see descriptions minimally at three different levels, the level of neurochemistry of emotional behavior (dopaminergic systems in the limbic system of the brain), the loci on the genome associated with these chemical and anatomical explanations, and moreover, the neurochemical and the genetic level are linked with behavioral measures of personality (Cloninger's TPQ).

7. Evaluation: reduction and levels in the genetic explanation of personality

We are now in a position to evaluate the accounts of reduction mentioned above against the backdrop of our case in behavioral genetics. Recall that the failure of classical reduction to obtain a plausible view on bridge laws led to two responses, autonomy/functionalism and NWR. In contrast, pluralistic views of explanation open the possibility of synchronous coexistence of theories at different levels (unavailable to the replacement–retention continuum advocated by NWR), and mutual feedback (unavailable to functionalists).

7.1. Classical reduction

Classical reduction assumes deduction and connecting principles as the main ingredients of reduction: the old reduced theory must be deduced from the new reducing theory, and the concepts of both must be connected by bridge laws. No

such cross-theoretical identities are in sight. One reason is that the classical model works only for completed (and formalized) theories. Schaffner (1993) suggests that the D-N model (in Schaffner's own modified version, the General Reduction Replacement model) might have some marginal role to play, as a picture of a completed (biomedical) science. Such a vision may be conceptually coherent and it may be useful to keep in mind as a possible future limit case of inquiry, but as a tool for understanding real life research it is of marginal significance.

So, classical reductionism is not a good account of the intertheoretic relations between genetic and behavioral theories. There are no cross-theoretic identities in the strict sense, and no deducibility of higher level from lower level. In our case study, personality theories are explanatorily extended by neurochemical and genetic findings on the one hand and the latter are informed by the former ("pleasure drug"). This correction would be impossible under (classical) conditions of consistency and meaning invariance. In fact, the change of meaning of concepts and theories in psychology, and the interpretation of anatomy and biochemistry in psychological terms is a major asset of interlevel synchronous investigation. It allows both top–down and bottom–up influences on theorizing. The classical model cannot account for such mutual correction and selection pressure.

7.2. Functionalism/autonomy

Multiple realization sank the project of classical reduction: if there is an open disjunction of identifications, psychological laws cannot in a meaningful way be connected cross-theoretically with genetic and neuroscientific concepts. The functionalists recognize no constraints from biology on psychological theorizing. Again, this cannot provide an adequate picture of the situation in behavioral genetics. One way out is to consider anything below the psychological level as implementational details, irrelevant for psychological explanation, and in no way constraining upper level theories (Fodor, 1981a,b). Obviously, in our case, the way temperament is determined by gene products is highly interesting and relevant, and not to be spurned as functionalists would have us do.

Functionalism is right in that some psychological theorizing can be done independently of physiological underpinnings. In our case study, Cloninger devised his personality scale from a functional psychological point of view. But as it happened, explanatory extension (Kitcher, 1981; Hardcastle, 1992) occurred: knowledge about brain systems, neuroanatomy and neurotransmitters can offer additional evidence that underpins the validity of these psychological personality constructs. This is neither autonomy nor elimination; rather functional theorizing is a first step towards two-way selection pressure, where the psychological theory is subject to cross-theoretic corroborations and criticism.

A brief remark may suffice for the underpinnings of functionalism, the notions of supervenience, multiple realization, and emergence. We can agree with Kim (1998) that these have heuristic and descriptive value (for example, it seems conceptually possible that the same phenotypic characteristics can be realized by different genotypes, but the current one is surely dependent on physical processes).

However, they don't provide explanations: supervenience does no more than to state the bare (contingent, empirical) fact (if it is a fact) that some higher-level phenomenon depends on a lower level; that requires rather than provides an explanation.

7.3. New Wave Reductionism

NWR succeeds in solving the problem of cross-theoretic identifications: it allows for the replacement of the old theory by a corrected version in the vocabulary of the reducing new theory. The disadvantage of such a model is that it can only account for theory succession, with some variations in the degree of retention or replacement of the old theory's ontology. In the genetics case there is no such diachronous replacement (or smooth reduction) of the psychological level by the genetic—rather synchronous coexistence of personality theory, of information on genetic coding, and on neurotransmitters.

7.3.1. Kim's functionalizing strategy. This strategy is a variety of reductionism. Its main thrust is to downplay the reality of functional explanations as lacking causal powers, and as no more than descriptions of the role played by real causal physical processes. Again, this model does not do justice to the interplay between personality, neurotransmitters and genomic information, and to the top–down and bottom–up co-evolution of these theories.

In our opinion, the mistake in the philosophy of mind has been to adopt the notions of universal causal laws from physics, and formulate the problems of reduction in terms of the deductive relations between them. It seems a much more viable strategy to borrow from biology, where pluralism, context dependence and historicity of explanations is the norm. This "disciplinary disintegration" (cf. van der Steen, 1993) tallies with explanatory pluralism that distinguishes the diachronic dimension of theory succession, reduction and replacement, from the synchronic dimension, allowing coexistence of partly overlapping theories. Coexistence is plausible if we realize that there are several distinct styles of explanations and domains in biology (van der Steen, 2000), that explanations depend on context and history (Rosenberg, 2001), and that no universal timeless laws are likely to be forthcoming (Schaffner, 1993). Philosophers with reductionist dispositions who claim that psychology must be understood as a form of biology seem to insufficiently realize the "middle level" nature of biological generalizations, and to construe biology along the lines of some kind of physics. The conclusion must be that synchronous coexistence and border traffic (including border disputes) will remain endemic in behavioral genetics. No replacement or theory succession seems in order between biochemistry, neuroscience and behavioral theories. Perhaps significantly, Bickle (2001) recently emphasized co-evolution as a feature of moderately smooth reduction; it remains obscure however how co-evolution fits in a (New Wave) reductionist picture, where only one dimension of intertheoretic relations is available, namely, the dimension of theory succession.

It could be argued perhaps that this line of reasoning confuses descriptive and normative concerns. The classical model is supposed to work on completed theories

(cf. Schaffner, 1993, on "clarified science"), and aims at a rational reconstruction, not at factual description of the process of inquiry. Since obviously in our case study above no mature theory is available, one could say that the D-N idea is not applicable in the case of biological psychology, and consequently, that the classical model is rejected prematurely. However, whenever a philosophical view does not fit real research, the former, not the latter has a problem. The classical model offers no resources for understanding explanatory practices, or for getting a handle on progress in interlevel explanations. The case of biological psychology presented above suggests that progress is made by relating co-evolving theories at different levels, and the philosophical task is to illuminate how and why that is the case.

8. Some remaining problems

A still insufficiently accounted-for aspect of the new behavioral genetics is that more than two levels of theorizing are at stake. Even a sophisticated model like HIT (McCauley & Bechtel, 2001), which advocates looking for partial and heuristic identifications between different domains and theories, envisages only two levels. How to understand selection pressure not only as mutual two-way feedback, but as at least three distinct levels in behavioral genetics is not obvious. It also involves a problem that has been glossed over until now: what is the weight of higher-level explanations relative to the mechanisms of its realizers? How can the notion of selection pressure and mutual constraints be fleshed out? For example, could psychological theorizing be overruled by genetics? HIT only suggests that such tensions will (usually) be heuristically productive.

Recalling the historic and contingent nature of evolutionary explanations, and the changing relations between function and mechanism, we could imagine another complication in modeling intertheoretical relations in biological psychology: the cross-theoretic heuristic identities might change. The consensus in biopsychology is that we may capitalize on homologies, in the case where temperament in mice was related to personality in man, both under control of loci on chromosome 11 that are supposedly homologous across mice and humans. Nothing, however, guarantees that mechanisms of approach and avoidance, aggression and fear (dependent as these are on environment and interaction with other organisms) remain unchanged in the course of evolution. Biological theories, including heuristic identification may, as Rosenberg (2001) argued, be historically contingent and variable.

Another issue where HIT provides little guidance is how to understand partial identities, i.e. low but robust covariances between phenomena at different levels. In our case study, only a small proportion of common variance is found between loci on the genome and behavioral and questionnaire scores. We lack criteria for converging evidence and for identification across theories. It might be that HIT focuses too much on positive heuristics and perhaps we should try to develop criteria for avoiding false positives (Schouten & Looren de Jong, 2001).

9. Conclusion

Accounting for the complex patchwork of related explanations in a multi-level science like biological psychology seems to require a more sophisticated and precise way of looking at levels than the existing ideas (both reductionist and non-reductionist) on explanation in the philosophy of mind, including Kim's functionalizing strategy, are able to provide. Above, some pluralistic proposals were sketched that might better fit interlevel relations in biological psychology. The typical "middle level" patchwork, located between overarching empty generalizations and local mechanisms, characteristic for biology, suggests partial bridges between domains, tentative identifications between theories and systems at different levels.

The point of this paper has been that the options in the wake of the failure of D-N reduction were too simple, and that capturing the interlevel relations in biological psychology should draw on the resources from philosophy of biology, especially where coexistence of partial and overlapping explanations is the case.

References

AMUNDSON, R. & LAUDER, G.V. (1994). Function without purpose: the uses of causal role function in evolutionary biology. *Biology and Philosophy*, *9*, 443–469.

BEATTY, J. (1996). Why do biologists argue like they do? *Philosophy of Science*, *64*, S432–S443.

BECHTEL, W.B. & MUNDALE, J. (1999). Multiple realizability revisited: linking cognitive and neural sciences. *Philosophy of Science*, *66*, 175–207.

BECHTEL, W.B. & RICHARDSON, R.C. (1993). *Discovering complexity: decomposition and localisation as strategies in scientific research*. Princeton, NJ: Princeton University Press.

BICKLE, J. (1998). *Psychoneural reduction: the new wave*. Cambridge, MA: MIT Press.

BICKLE, J. (2001). Concepts structured through reductions. *Synthese*, *130*, 123–133.

BRANDON, R.N. (1990). *Adaptation and environment*. Princeton, NJ: Princeton University Press.

CHURCHLAND, P.M. (1979). *Scientific realism and the plasticity of mind*. Cambridge: Cambridge University Press.

CHURCHLAND, P.M. (1981). Eliminative materialism and the propositional attitudes. *Journal of Philosophy*, *78*, 67–90.

CHURCHLAND, P.M. (1989). *A neurocomputational perspective: the nature of mind and the structure of science*. Cambridge, MA: MIT Press.

CHURCHLAND, P.M. & CHURCHLAND, P.S. (1994). Intertheoretic reduction: a neuroscientist's field guide. In R. WARNER & T. SZUBKA (Eds) *The mind–body problem* (pp. 41–54). Oxford: Blackwell.

CLONINGER, C.R., ADOLFSSON, R. & SVRAKIC, N.M. (1996). Mapping genes for human personality. *Nature Genetics*, *12*, 3–4.

DARDEN, L. & MAULL, N. (1977). Interfield theories. *Philosophy of Science*, *44*, 43–46.

DENNETT, D.C. (1991). Real patterns. *Journal of Philosophy*, *88*, 27–51.

DENNETT, D.C. (1995). *Darwin's dangerous idea*. London: Allen Lane.

EBSTEIN, R.P., NOVICK, O., UMANSKY, R., PRIEL, B., OSHER, Y., BENNETT, E.R., NEMANOV, L., KATZ, M. & BELMAKER, R. (1996). Dopamine D4 receptor (D4DR) exon III polymorphism associated with the human personality trait of novelty seeking. *Nature Genetics*, *12*, 78–80.

ENC, B. & ADAMS, F. (1992). Functions and goal directedness. *Philosophy of Science*, *59*, 635–654.

FEYERABEND, P. (1968). How to be a good empiricist—a plea for tolerance in matters epistemological. In P.H. NIDDITCH (Ed.) *The philosophy of science* (pp. 12–39). Oxford: Oxford University Press.

FODOR, J.A. (1981a). The mind–body problem. *Scientific American*, *244*, 124–132.

FODOR, J.A. (1981b). Special sciences. *Representations: philosophical essays on the foundations of cognitive science* (pp. 127–145). Hassocks: Harvester.

FODOR, J.A. (1990). Making mind matter more. *A theory of content and other essays* (pp. 137–159). Cambridge, MA: MIT Press.

GOULD, S.J. & VRBA, E.S. (1998). Exaptations—a missing term in the science of form. In C. ALLEN, M. BEKOFF & G. LAUDER (Eds) *Nature's purposes: analyses of function and design in biology* (pp. 519–540). Cambridge, MA: MIT Press.

HAMER, D. & COPELAND, P. (1998). *Living with our genes*. New York: Doubleday.

HARDCASTLE, V.G. (1992). Reduction, explanatory extension, and the mind/brain sciences. *Philosophy of Science*, *59*, 408–428.

HEMPEL, C.G. (1965). Aspects of scientific explanation. In C.G. HEMPEL (Ed.) *Aspects of scientific explanation* (pp. 331–496). New York: Free Press.

KIM, J. (1992). Multiple realization and the metaphysics of reduction. *Philosophy and Phenomenological Research*, *52*, 1–26.

KIM, J. (1993). *Supervenience and mind*. Cambridge: Cambridge University Press.

KIM, J. (1998). *Mind in a physical world*. Cambridge, MA: MIT Press.

KIM, J. (1999). Making sense of emergence. *Philosophical Studies*, *95*, 3–36.

KITCHER, P. (1981). Explanatory unification. *Philosophy of Science*, *48*, 507–531.

KUHN, T.S. (1970). *The structure of scientific revolutions*. Chicago: Chicago University Press.

MAYR, E. (1988). *Toward a new philosophy of biology*. Cambridge: Cambridge University Press.

MCCAULEY, R.N. (1996). Explanatory pluralism and the coevolution of theories in science. In R.N. MCCAULEY (Ed.) *The Churchlands and their critics* (pp. 17–47). Oxford: Blackwell.

MCCAULEY, R.N. & BECHTEL, W. (2001). Explanatory pluralism and the Heuristic Identity Theory. *Theory and Psychology*, *11*, 736–760.

MCGUE, M. & BOUCHARD, T.J. (1998). Genetic and environmental influences on human behavioral differences. *Annual Review of Neuroscience*, *21*, 1–24.

MITCHELL, S.D. (1995). Function, fitness and disposition. *Biology and Philosophy*, *10*, 39–54.

NAGEL, E. (1961). *The structure of science*. London: Routledge.

OPPENHEIM, P. & PUTNAM, H. (1958/1991). Unity of science as a working hypothesis. In R. BOYD, P. GASPER & J.D. TROUT (Eds) *The philosophy of science* (pp. 405–428). Cambridge, MA: MIT Press.

PLOMIN, R. & DEFRIES, J.C. (1998). The genetics of cognitive abilities and disabilities. *Scientific American*, *278*, 62–69.

PLOMIN, R., DEFRIES, J.C., MCCLEARN, G.E. & RUTTER, M. (1997). *Behavioral genetics*. New York: Freeman.

PUTNAM, H. (1961). Minds and machines. In S. HOOK (Ed.) *Dimensions of mind* (pp. 221–231). New York: Collier.

ROSENBERG, A. (1994). *Instrumental biology or the disunity of science*. Chicago: Univeristy of Chicago Press.

ROSENBERG, A. (1997). Reductionism redux: computing the embryo. *Biology and Philosophy*, *12*, 445–470.

ROSENBERG, A. (2001). Reductionism in a historical science. *Philosophy of Science*, *68*, 135–163.

SCHAFFNER, K. (1993). *Discovery and explanation in biology and medicine*. Chicago: Chicago University Press.

SCHOUTEN, M.K.D. & LOOREN DE JONG, H. (1999). Reduction, elimination, and levels: the case of the LTP-learning link. *Philosophical Psychology*, *12*, 239–264.

SCHOUTEN, M.K.D. & LOOREN DE JONG, H. (2001). Explanatory pluralism and heuristic identification: some explorations in behavioral genetics. *Theory and Psychology*, *11*, 796–807.

SHORS, T.J. & MATZEL, L.D. (1997). Long-term potentiation: what has learning got to do with it? *Behavioral and Brain Sciences*, *20*, 597–655.

SOBER, E. (1985). Panglossian functionalism and the philosophy of mind. *Synthese*, *64*, 165–193.

SOBER, E. (1999). The multiple realizability argument against reductionism. *Philosophy of Science*, *66*, 542–564.

VAN DER STEEN, W.J. (1993). Towards disciplinary disintegration in biology. *Biology and Philosophy*, *8*, 259–275.

VAN DER STEEN, W.J. (2000). *Evolution as natural history*. Westport: Praeger.

WATERS, C.K. (1994). Genes made molecular. *Philosophy of Science*, *61*, 163–185.
WATERS, C.K. (1998). Causal regularities in the biological world of contingent generalizations. *Biology and Philosophy*, *13*, 5–36.
WEINBERG, S. (1993). *Dreams of a final theory*. London: Vintage.
WOUTERS, A.G. (1999). *Explanation without a cause*. Utrecht: Zeno.